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GLACIATION IN AUSTRALIA.

BY T. S. HALL, CASTLEMAINE, VICTORIA, AUSTRALIA.

EVIDENCES of one or more glacial epochs are plainly visible in Australia, and the more closely is study directed to the subject, the more widely spread are the glacial deposits found to be. As long ago as 1861 Selwyn, then the Director of the Geological Survey of Victoria, noted that several conglomerate beds in various parts of the colony were evidently the results of ice-action, although no striated stones were visible. In 1877 Professor Tate of Adelaide announced the discovery of a glaciated surface near that city, and toward the close of 1889 Mr. E. J. Dunn found grooved stones in Victoria. Since then the Mining Department of Victoria has issued a report by Mr. Dunn of one of these conglomerate beds near Heathcote. The deposit covers about 36 square miles, and consists of a base composed of dark indurated clay, through which are scattered masses of rocks of various kinds—granites, syenites, gneisses, schists, quarzites, slates, shales, conglomerates, etc., etc. Many of the granites are not known in Victoria, *in situ*, and their origin can only be guessed at at present. In one or two places glaciated surfaces are seen and the striæ run north and south. The largest "erratic" known is a block of extra-Victorian granite, weighing about 30 tons. The thickness of the beds is estimated at about 400 feet. The bed rock is of Lower Silurian age, and is tilted at a high angle. Intercalated beds of sandstone occur in places, and show the deposit to be still nearly horizontal. In a paper recently read before the Royal Society of Victoria, Messrs. Officer and Balfour record grooved pebbles, "contorted till," and glaciated surfaces near Bacchus Marsh. The deposit there has, moreover, been heavily faulted.

The age of the Victorian deposits has not been precisely fixed as yet. At Bacchus Marsh the beds are overlain by fresh water sandstones containing *Gangamopteris, Schizoneura*, and *Zeugophylletes* (?), and which are stated by M'Coy to be of Triassic age. The age of the glacial beds is then perhaps Palæozoic. No fossil remains have as yet been found in the glacial beds themselves, but doubtless careful washing of the clays will yield evidences of life, as it has done in other countries. Small outliers of these beds are found widely scattered over the colony, from north to south, and on both sides of the Dividing Range. They extend into New South Wales, and may be looked for, Dunn says, at the foot of the western slopes of the Great Divide. Similar beds occur on the eastern edge of the great Queensland Downs

Mr. Dunn draws a parallel between these beds and the Dwyka conglomerates of South Africa, which are of Triassic age. If the parallel prove a good one, then we have evidence of an enormous extent of glaciation at the close of the Palæozoic or the beginning of the Mesozoic, extending nearer the equator than that of the Northern Hemisphere, during the last great ice age. The South Australian beds at Hallett's Cove, near Adelaide, before alluded to, are of Tertiary age. Here the glacier path can be traced for about two miles, and moraine *débris* is in abundance. Traces of more recent glacial action are recorded from the neighborhood of Mount Koscius Ko, but these are of local origin, and are perhaps due to a greater elevation of the region, as no glaciers exist in Australia at the present time.

SECRETS OF THE ATMOSPHERE.

BY H. A. HAZEN, WASHINGTON, D.C.

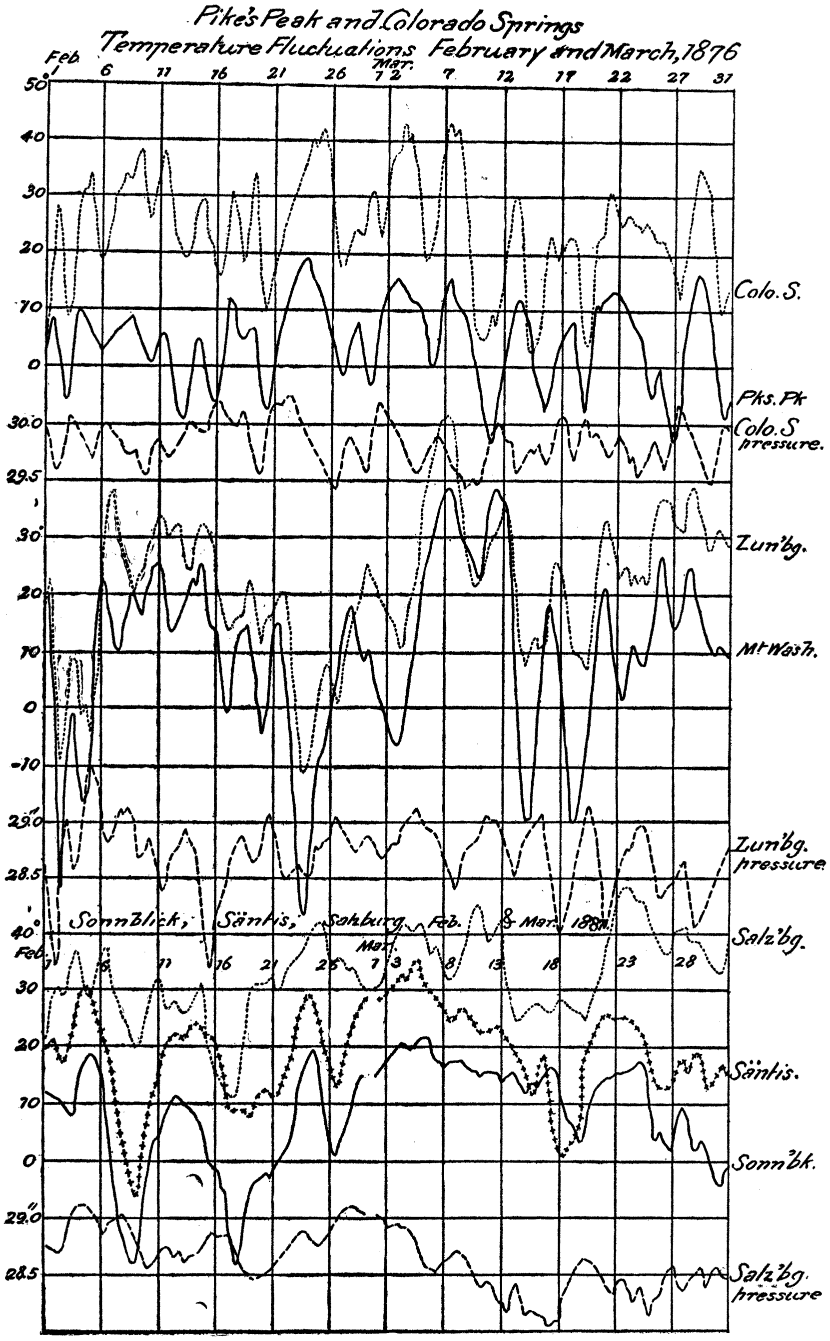
IN the March number of the *American Meteorological Journal*, Professor Harrington treats at some length the subject, "Exploration of the Free Air," and urges the great necessity of such an enterprise. For more than eight years the present writer has insisted that by no other means will it be possible to set the science of meteorology upon a firm basis and rid it of mere speculations and theories which too often have served to prevent its advance in the past. Professor Harrington quotes a graphic description of an experience of the aeronaut Wise, in which he seemed to be thrown or attracted back and forth in an ominous thunder-cloud. Several such have been described by aeronauts, who unfortunately had not the instruments requisite to give very necessary information in these cases and to make them of avail in a scientific study. The description of these mysteries make us long for something more tangible and definite.

To my mind there is no research of so great importance in the whole range of science as that of a few well conducted ascensions, with accurate instruments, in the midst of a rain-storm and on all sides of a low area. Ordinarily, balloon voyages have been made during clear weather and for the benefit of a great assemblage, so that this field, or the problem of ascertaining the secrets of the air, has been almost entirely neglected up to the present. A single illustration will show the extreme necessity of systematic work in this line.

It may not be generally understood that there has been an extraordinary revolution in meteorology within the past six years. During this revolution the whole convection hypothesis of storm generation, without the least doubt the most important of all the theories of orthodox meteorology, has been attacked and completely overthrown. The significance of this defeat cannot be exaggerated and should be fully set forth. The convection theory is fully advanced in Professor Ferrel's last book, published in 1890, "A Popular Treatise on the Winds," p. 228. "On account of the non-homogeneity of the earth's surface, comprising hills and valleys, land and water, and dry and marshy areas, all with different radiating and absorbing powers, and also on account of the frequently irregular and varying distribution of clouds, it must often happen that there are considerable local departures of temperature from that of the surrounding parts; and if it should so happen, as it frequently must, that this area is of a somewhat circular form, and the air has a temperature higher than that of the surrounding part of the atmosphere, then we have the conditions required to give rise to a vertical circulation, with an ascending current in the interior, as described above. But unless there is some source of heat by which this interior higher temperature is kept up, this circulation soon ceases, for the interchange of air between the interior and exterior parts of the air comprised in the circulation tends to continually reduce the difference of temperature upon which the circulation depends, and to bring all parts to the same temperature. . . . In the case of a moist atmosphere with the unstable state for dry air, we have the same energy for originating and maintaining a vertical circulation as in the case of dry air, with the additional energy of all the latent heat of the aqueous vapor set free in its condensation

in the ascending current, and this latter is a continuous source of energy as long as moist air is being drawn in from all sides to supply this current."

These are the careful words of one who devoted more than thirty years to a most thorough study of this whole question. I am well aware that certain disciples of Professor Ferrel are trying to take advanced ground on this vital question, and are striving to show that we may have a storm with lower temperature in its upper portion, but it seems to me this is a fatal error, and if persisted in must overthrow the convection hypothesis. I do not see how there can be any middle ground in this matter. If we accept Ferrel's views, we must stand by the convection hypothesis. It is well known that I have taught for many years that the convection hypothesis is disproved by the most convincing facts, and cannot possibly be sustained. It is none the less true, however, that the temperature in our storms up to great heights is vastly higher than that of the surrounding air, and in our high areas it is vastly less. These facts are absolutely established by observations on mountain-tops. I give here curves showing the fluctuation of temperature at the base and summit of the following stations: Pike's Peak (14,134 feet, highest meteorological station in the world), and Colorado Springs (5,950 feet); Mount Washington (6,279 feet), and Lunenburg (1,100 feet); also at Sonnblick (10,170 feet), and Salzburg (1,434 feet), in Austria. In the last set of curves I have added Säntis (8,202 feet), situated 165 miles west of Sonnblick. Any one studying these curves must be convinced that the temperature fluctuations are precisely the same at the summit and base of each high station. Now it



is a universal law that on the approach of a storm the temperature rises at sea-level, and with a high area there is a decided fall, especially in the colder months. This law is abundantly borne out in these curves, for a comparison between the pressure curve (broken) at the base and the temperature curve (dotted) shows opposite phases between the two, and the temperature maxima and minima occur earlier at the summit than the pressure minima and maxima at the base. This is shown most clearly at the stations in the United States, but it can be seen also at the Austrian stations.

To my mind, it is impossible to conceive of a normal storm, seeming to move at the rate of 30 or 40 miles per hour along the earth, which does not have a higher temperature up to a height of at least 5 miles, and probably much higher. The reverse of this must also be true in the case of a high area. In fact, I utterly fail to see how an area of high pressure can have a rapid motion unless in its centre there is denser air, brought about by a greater degree of cold, and this, as I understand it, is exactly the view of Professor Ferrel. These views were generally held up to 1886, and I am not aware that any one disputed them, except as regards the pure convection hypothesis.

In 1886 M. Dechevrens wrote a paper in which he tried to show that the usual law of the relation of pressure and temperature at the earth's surface was exactly reversed at the height of Mount Washington (6,279 feet), and that at that point a fall in temperature occurred with a fall in pressure, and *vice versa*. This was a most astonishing result, and seemed to disprove the whole convection hypothesis. His research consisted in studying the pressure and temperature fluctuations at the summit without any reference to the passage of storms or high areas at the base.

In this study he ignored the fact that after the passage of a storm the cold wave following would tend to contract the air below the summit, and hence the pressure would continue to fall, and the minimum would not be reached till some time after the passage of the storm at the base. Exactly the reverse conditions would be found on the passage of a high area. A full analysis of this proposition, with curves showing these effects, will be found in Annual Rept., Chief Signal Officer, 1882, pp. 897–902.

In addition to this difficulty in comparing these fluctuations directly, there is another almost as serious, which lies in the fact that the maximum and minimum points in the temperature oscillations occur several hours earlier at the summit than they do at the base. It will be seen readily that both these conditions would tend to bring the minimum of temperature at the same time as the minimum of pressure at the summit. How closely these conditions of temperature at the base agree with those at the summit we have already seen perfectly demonstrated in the curves given above. This paper of Dechevrens was translated and published with comments dissenting strongly from the views advanced in the *American Meteorological Journal*, August, 1886, pp. 297–314.

It is probable that these researches would have attracted very little attention had it not been for a study by Dr. Hann of observations at Sonnblick (10,170 feet) on almost exactly the same lines as those pursued by Dechevrens, and with the same result. The first paper by Dr. Hann was published in April, 1887, in the *Meteorologische Zeitschrift*, and this was followed by others in the same and other journals, the last and most thorough research of all, of 86 pages, appearing in April, 1891. In this study the conditions were still farther complicated, from the fact that Sonnblick lay in southern Europe, where very few, if any, normal storms or high areas pass. High areas, with a pressure of 30.7″, have been known to hover over this region for three weeks at a time. Such conditions are unheard of in the United States, and their effect can be at once recognized by comparing the fluctuations in the lowest series of curves with those in the other two in the diagram already given. It is easy to see that under such conditions the stagnant air above would become abnormally and cumulatively heated day by day, while the lower air in a clear sky would be abnormally cooled, and there would at times appear to be a reversal of temperature. With all these disadvantages, however, it will be seen that the temperature curve at the base of Sonnblick, in its larger oscillations, agrees almost exactly with that at the summit.

One of the most remarkable results found by Dr. Hann was that the maximum and minimum points of temperature lag a day behind those of the maximum and minimum air-pressure. It seems almost incredible that such a deduction could have been made. It seems as if it could only have been by confusing or comparing the minimum air-pressure oscillatian of a storm with the minimum temperature oscillation of the following high area. An examination of the diagram already given brings out this fact most clearly. In nearly every case, both at Pike's Peak and Mount Washington, the minimum of air-pressure occurs a day earlier than the minimum of temperature accompanying the succeeding high area. I am sure that no one will make this mistake on studying the diagram. The maximum of temperature accompanying a storm occurs about a day earlier than the minimum of pressure accompanying the same storm. The minimum of temperature accompanying the succeeding high area has nothing whatever to do with the previous minimum of pressure, and a proper study of the diagram shows at once the truth.

It seems to me my position in this matter is brought out most clearly and distinctly in the last paper by Dr. Hann, of 86 pages, in April, 1891. Speaking of fluctuations of pressure and temperature at p. 367, Dr. Hann says: "Für die Erdoberfläche sind dieselben seit Langem bekannt." "For the earth's surface have these been for a long time well known." I am sure that every one will admit that at the earth's surface as a storm comes up the temperature rises and is the highest during the storm. As a high area advances, the temperature falls, and is generally vastly lower during such pressures than during storms, especially in the winter season. On page 370 Dr. Hann gives the temperature conditions during barometer maxima, and on page 375 the conditions with minima. The sea level station at the base of Sonnblick was Ischl, and I give here the temperature in both maxima and minima during the colder months.

Temperature at Ischl, Cold Months.

Barometer Reading.	Maximum.	Minimum.	Difference.
	Centigrade.	Centigrade.	Centigrade.
October	8.4°	6.5°	1.9°
November	2 8	2.4	0.4
December	– 2.6	– 3.3	0.7
January	– 1.5	– 0.3	– 1.2
February	1.0	– 4.8	5.8
March	4.0	– 5 1	9 1
Mean	2 0	– 0.8	2 8

This table shows that at the base of Sonnblick during every month except one the temperature is higher in a maximum barometer reading than in a minimum, and the average difference is 2.8° C., or 5.0° F. This exact and marked reversal of the universal law is very significant and proves conclusively that there has been either a most serious error in studying or selecting out the different cases, or that the universal law does not hold for this region.

It would seem that Dr. Hann himself now recognizes the difficulty in using these records, for he says, in a paper on Ben Nevis (4,406 feet), *Meteorologische Zeitschrift*, December, 1892, p. 457: "Wie Herr Buchan schon in seinem ersten Bericht hervorhebt, zeigt auch der Ben Nevis sehr häufig die Erscheinung hoher Temperatur und grosser Trockenheit selbst mitten im Winter, sobald er in das Gebiet eines Barometermaximums zu liegen kommt. Es treten dann auch öfter die sogenannten Temperaturumkehrungen auf, was hier besonders bemerkenswerth ist wegen der freien Lage von Fort William am Meere, welche eine Stagnation kalter Luft an der unteren Station ausschliesst." "As Mr. Buchan showed in his first report, very often Ben Nevis has the appearance of higher temperature and greater dryness in winter as soon as a barometer maximum lies in that region. It shows then often the so-called temperature reversal, which here is especially remarkable because of the free position of Fort William on the sea, which prevents a stagnation of cold air at the lower station." The question seems to be clearly set forth in these words, and it is probable that the advocates of the view that in the centre of our high areas there is a rise in temperature at some height above the earth will be willing to stand or fall by the proofs at Ben Nevis.

The date of this abnormal heat was Dec. 31, 1883.[1] Another occasion was on Nov. 18, 1885, and a third on Feb. 5, 1886. These are the only marked cases from December, 1883, to February, 1886, though there were minor cases of no importance on Jan. 16 and Dec. 22, 1884, and on Nov. 10, 1885. I have made a careful search of all the published observations for maximum barometer readings in the four cold months, and have found 70 cases. That is to say, out of 70 cases, only 3 show a marked departure from the law that there is the same oscillation of temperature at the summit as at the base of Ben Nevis. But this is not all. On Dec. 31, 1883, the motion of the high area was quite slow and the wind on Ben Nevis almost a calm, thus causing a stagnant air. On Dec. 28, or three days earlier, the temperature at the summit began falling, and in 24 hours it had fallen nearly 22° F., or *more* than at the base in the same time. This shows that the usual law was acting even in this case and that the subsequent rise was due to an abnormal condition and not to the fact that the temperature was higher in the centre of the high area than on either side.

In the other case cited by Dr. Hann on Nov. 18, 1885, the conditions were exceedingly abnormal, as the high area moved from the *east* toward the *west*. It would be impossible to reason as to the ordinary temperature conditions in a high area from such a case. It is an interesting fact that in the latter case the oscillation of temperature at the summit was precisely the same as at the base, except that the fall and rise at the summit was a little greater than at the base, and it took place about 24 hours later, instead of earlier as is usually the case. The usual law of lower temperature in the centre of a high area is abundantly borne out at Ben Nevis, and I have found the reverse law in the centre of a low area also true at that station.

I have thus dwelt at some length upon these studies for the reason that they have been largely accepted by European meteorologists and have served to overthrow nearly every hypothesis that has been regarded invulnerable in the past. Is there not here the best proof in the world of the extreme need of an exploration of the atmosphere at the seat of these disturbances? Meteorology needs, above all things else just at present, a full and complete setting forth of the facts to be gleaned in the upper atmosphere. An array and study of these facts would give us a good foundation on which to lay the corner-stone of a good and exact science. It would be of inestimable value in forecasting the weather and in removing our ignorance, which is so serious a drawback at present.

We do not know positively the simplest conditions in the atmosphere. Glaisher once left London in a pouring rain and emerged into clear sky after rising only 800 feet. At another time he found rain falling in a cloud 15,000 feet high. In this country no rain has been observed in balloon ascensions above 9,000 feet, and it is probable that a large part of our rain forms at a height less than 6,000 feet. We do not know the thickness of a rain-cloud nor its temperature. Some think the temperature must be higher than that of the surrounding air, else the storm would quickly cease; others think that no rain can form unless the temperature is lower than the outside air. Our books are full of speculations as to the dynamic heating of the air and the conditions needed to originate and maintain our storms and high areas. The evidence seems quite clear that all these theories, often contradictory among themselves, would not account for a tithe of the energy displayed, and an exploration is needed to determine this fact, or to establish the truth.

Is there an ascending current in our storms, or a descending one in our high areas? These are theories of the deepest interest. The evidence seems to show that there is not a transfer of an air-mass in any direction, either up or down or horizontally, in our storms or high areas. Professor R. H. Scott, after giving all possible sources of rain formation, decides that the only one that can be maintained on theoretical grounds is that rain is formed in an ascending current of warm, moist air. A determination of this question would be of inestimable value in all studies and researches as to the natural or artificial formation of rain.

[1] Misprinted 1884.

In several ascensions in this country it has been found that there seem to be rather definite layers of moisture even in a clear sky. Sometimes two layers have been found at different heights. These would seem to be exceedingly significant facts. Do these layers serve as conductors for electric currents, as seems to have been very guardedly stated by Professor Loomis? How do these layers thicken as a storm comes up, or, rather, is the thickening process a precursor to the storm? Does this thickening in a certain definite direction show in what direction the storm will subsequently move, or is it caused by the conditions accompanying the storm? Do these layers rise or fall, or what is their movement under different atmospheric conditions?

What relation does the dust in the atmosphere bear to these layers? Is there an increase of dust in definite layers? Is dust needed to produce this thickening? It seems to me the careful and painstaking investigations of Professor Barus in cloud condensation must bear valuable fruit as soon as he turns to the ordinary conditions in our storms, and for this the study can be prosecuted only with great difficulty, except in nature's own great laboratory.

A serious drawback in the past to successful balloon exploration has lain in the lack of suitable instruments. Professor Glaisher often took up instruments enough to stock a meteorologic observatory, and in a single ascension once broke nearly $500 worth. What is needed is an instrument that can be read very quickly, once a minute if possible, and, at the same time, do its work very accurately. A sensitive aneroid will give the pressure, and a sling psychrometer will give the moisture conditions. Various rather singular objections have been raised to this instrument. One is that it will give 5° too high temperature under strong insolation. This experiment has been tried, and it is known that under the strongest insolation possible the temperature will be less than .8° higher in the sun than in the shade. Another objection raised has been that it will give a lower relative humidity in bright sunshine than in shade. This is entirely wrong, because the muslin coating of the wet bulb is a vastly better absorber of heat than the bright bulb, and hence, if anything, in bright sunshine the relative humidity must be higher than in shade. It is also said that the heat of the balloon will tend to raise the temperature of the sling thermometer because it cannot be used far enough away from the basket. In a comparison between the sling thermometer and another so-called standard (aspiration thermometer) the greatest difference between the two occurred when the balloon was moving horizontally, and the least when the balloon was ascending most rapidly, so that this objection utterly fails. The true criterion of an accurate instrument is that it shall give the same temperature of any stratum in a rapid ascent and descent, and this is fulfilled in a marked degree by the sling psychrometer. I have used this instrument for over eight years and in five balloon voyages, and am satisfied that it is a perfect instrument and one that responds at once to any demands put upon it.

The expense of ballooning in the past has been enormous, and a serious drawback to its prosecution. One is amazed to read that in certain high ascensions, to five miles and over, the balloon of 90,000 cubic feet capacity was filled plump full, thus necessitating the carriage of about a ton and a half of ballast. This ballast had to be poured out and more than half the gas wasted before reaching the height desired. It is no wonder that the aeronaut was completely exhausted with his labors with the ton of ballast. All this gas that had to flow out, because of expansion, was a dead loss, say, $150 for each ascension, and after landing the remaining gas was emptied. All of this expense can be avoided, as I am firmly convinced. It is well known that if a balloon leaves the earth at all, it will rise till the envelope is plump full. If the balloon will rise when two-fifths full of gas, it will continue to do so till it has reached more than five miles, the limit desired at present, though there is no reason why ultimately we may not ascend to the extreme limit to which hydrogen may carry us by the use of a pneumatic cabinet. It is proposed to employ a small balloon with hydrogen gas. A balloon of 20,000 or 30,000 cubic feet will easily carry two men when half full, and the enormously less labor of handling it, as com-

pared with that of handling one of 100,000 cubic feet, can hardly be estimated. The risk in a high wind of the smaller balloon is vastly less than of the larger. Every way the smaller balloon presents advantages over the larger. The first cost of a balloon of 20,000 cubic feet would be $600. The cost of a half charge of gas need not be over $30, and may be less. It is hoped that the balloon will be sufficiently tight to hold its gas for a long period. In Europe balloons have been made with gold-beaters' skin that have leaked only ¼ of 1 per cent in 24 hours. I think the leakage of a cloth balloon when properly made need not be over 4 or 5 per cent, but the figures in this country are exceedingly meagre and unsatisfactory. After an ascension it will be a very simple matter to conserve the gas, and, if wished, an addition may be made at the landing-point with gas from a flexible holder, which may be easily transported from point to point.

An interesting problem presents itself as to the behavior of the gas in a rapid ascent or descent. Theory indicates that in a rapid expansion dry gas will cool 1° F. in 186 feet ascent, so that at 25,000 feet the temperature would be about 130° lower at the centre of the balloon than at the outside air, provided the ascension was quick enough to prevent the heat from striking in. Now experience in balloon ascents shows that the gas in a balloon is invariably *warmer* than the outside air. Exactly the reverse is true in a rapid descent, both as regards theory and practice. Whether this is due to the fact that the envelope retains its heat or not, it still remains that we have here apparently a means of making our ascensions with the loss of little or no gas at the valve. At the highest point our gas will be cooled and lose its buoyancy, which allows a fall in the balloon, which is always greatly accelerated as we approach the earth, and after landing the balloon may be anchored till the sun's heat has warmed the gas, which will enable another trip with the same gas.

The risk in such ascents has been greatly exaggerated by some from the serious and often fatal accidents that have attended jumping with parachutes and ascending in hot-air balloons. The modern balloon, with its very long drag-rope and rip-cord, are very safe. Even in case the balloon should burst, the envelope catches in the netting and acts like a parachute in breaking the fall. Mr. Wise, the veteran, once ascended to the height of a mile and purposely exploded his balloon in order to show that there was no great risk in such an adventure. In one case, Mr. King and a married couple were in a balloon which exploded at the height of a mile, and without serious consequences It should be noted that a new balloon will not explode. Glaisher reports having ascended with a balloon full of gas at the rate of 4,000 feet per minute; this was a remarkable feat. It is not the intention to ascend faster than 1,000 feet per minute, and at this rate the danger of bursting is almost nothing.

Some may think that such observations may be made at vastly less risk, expense, and discomfort on mountain tops. Undoubtedly there are some observations of temperature that may be made in this way, but even in this case we cannot tell just what effect the summit will have. Observations of rainfall, clouds, electricity, etc., are entirely impossible on mountain-tops, for the reason that these have a peculiar action of their own entirely different from that of the free air. It seems probable that the mountain acts like a point in the atmosphere from which there is a continuous discharge of electricity, as in the case of a point on the conductor of an electric machine.

The exploration of the atmosphere cannot be carried on in Europe to as good advantage as in this country, for the reason that they do not have the normal low areas and high areas travelling at some velocity that we have. The conditions of the atmosphere are so different in the two countries that we must make our own researches. I trust I have shown the great need of such exploration. I know of no endowment of $5,000 or $10,000 that would pay so rich and immediate a harvest as this for ballooning. Thousands are spent in visiting the inhospitable north, while a field just at our hands, which may be explored at vastly less expense and risk, and which promises immeasurably greater returns, is left unexplored and unvisited.

March 31, 1893.

LOSS OF DRY MATTER BY THE SPROUTING OF CORN-SEEDS.

BY E. H. FARRINGTON, CHEMIST, AGRICULTURAL EXPERIMENT STATION, CHAMPAIGN, ILL.

SEEDS of the corn-plant were placed in damp cotton and left to sprout in the dark for nine days. Four of these seeds partially sprouted, then moulded, failing to develop further. They lost by this treatment 9 to 18 per cent of the dry matter in the original seed.

Two seeds, under the same conditions for nine days, sprouted and developed a corn plant. The root and stem of these plants each measured two to three inches, and their weight was from three to three and one-half times that of the original seed. It was found, however, that when the water was dried out of these young plants the dry matter in them was 20 and 31 per cent less than the seed contained.

Several estimations were made of the per cent of water and dry matter in a sample of corn. These results were used for estimating the weight of water and dry matter in the corn which was taken from the same sample and sprouted.

Details of Weights in Grams.

	Dry Matter.	Water.	Total.
Weight of seed before sprouting	0 271	0.042	0.313
After nine days sprouting in damp cotton, plant with seed attached, weight	0.187	0.747	0.934
Gain or loss of plant over seed	− 0.084	+ 0.7 5	+ 0.621
Per cent gain or loss was of weight in the seed	− 31.0	+ 1680	+ 198
Duplicate observation gave	− 19.8	+ 1945	+ 239

This shows that in sprouting the white plant had taken up water but lost in dry matter.

This experiment was repeated June 3, 1892, by sprouting the seed in the soil of a corn-field instead of cotton. One week after planting, four of the plants were dug up. They were about two inches above ground and had two green leaves. The shell of the seed still clung to the plant. The root was about five inches long, making a total length of about ten inches from tip of leaf to end of root.

The weight of these green plants, free from soil, was about four times that of the seed planted, but they contained from 58 to 79 per cent only of the dry matter in the original seed.

During the week these seeds were growing the climatic and soil conditions were ideal for corn.

Details of Weights and Measurements.

Plant No.	Weight in Grams.			Measurement of Plant. Inches.			Dry Matter.	
	Seed.							
	Dry Matter.	Total.	Green Plant.	Tip of Leaf to Seed.	Above Ground.	Roots.	In Plant.	Per cent of that in Seed.
1	0.416	0.479	1.633	4	2	5	0.331	79.3
2	0.357	0.412	1.447	4½	2	5	0.210	58.8
3	0.347	0.450	1.549	3½	2	4	0.273	78.6
4	0.393	0.457	1.454	1½	3½	4	0.310	78.2

Two weeks after the seed had been planted, five plants were cut at the surface of the soil, and the weight and measurements of each plant above ground was compared with the weight of the seed. This shows that corn-plants, having a height of ten to fourteen inches above ground, weighed when green four to eight